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ON THE

ELECTRICAL EFFECTS

PRODUCED

BY FRICTION BETWEEN BODIES.

By J. DE LUC, Esq. F. R. S.

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*Between Bodies.**

By J. A. De Luc, Esq. F. R. S.

IN my former papers I have communicated to you my remarks on Dr. Maycock's electrical system; and I come now to his paper in your Journal, No. 144, concerning the *production of electrical excitement by friction.* This paper concludes by the following very judicious remark, which induces me to offer here to him my ideas on the same subject. Reference to former papers.

"It will afford me much pleasure (says Dr. Maycock) "should these observations call the attention of your readers to "the *theory* of *electrical excitement.* I trust that, while we are "successfully employing the powers of electricity in chemical "analysis, we shall not altogether neglect to investigate the "means by which these powers are called forth, and the laws "by which their action is regulated. It has, with much in-"justice, been objected to *theoretical* pursuits, that they lead "to none of the *practical* advantages, which interest the happi-"ness of society. The remark is indeed true, if applied to "particular discoveries; but these are to be considered only as "the elements from which physical science first took its ori-"gin, and by which it is daily nourished and supported. Let "it never be forgotten, that our most perfect instruments, "those which promote no less our comfort, than they tend to "advance our intellectual improvement, are the invaluable "fruits of philosophy." Journ. vol. XXXI, p. 309. Theory of electrical excitement deserves attention.

1. In quoting this passage with approbation, I cannot, Sir, but express again my regret, that Dr. Maycock appears to have no knowledge of my papers in your Journal; for they might Effects of friction before examined.

* From Nicholson's Philosophical Journal, vol. XXXIII.

have

have given him the opportunity of useful examinations between us. For instance, in your No. 126, for January, 1811*, is my paper under the title of *Experiments, showing the effects of Friction between bodies*; which experiments might have afforded him what he wishes to find in your readers, viz. some *remarks* to be compared with his *theory*. But if he reads my present paper, there will be only a little time lost, and the examination may now be effected more directly between us in your Journal.

Dr. Maycock's opinion of these effects.

2. Dr. Maycock's system on the effects of *frition* is derived from his opinion, which, in my former papers, I have proved to be unfounded, viz. that the *electrical* effects produced by the association of *two* proper *metals* appeared only when they came to be *separated*. Had Dr. Maycock known these papers, he certainly would have thought it proper to answer me, before he took his system as a principle in explaining the effects of *friction*, as he does thus in vol. XXXI, p. 305. "It "must be obvious, that, while we are drawing one body over "another, a number of points in the surface of the *rubber* are "first brought into *contact* with a corresponding set of points "in the surface of the *body rubbed*; that they are then *sepa-* "*rated* from them, and brought into *contact* with another set of "points; and so on, until the one body has passed entirely over "the other. Now, at each *separation*, if the bodies be of different "kinds, whether conductors or nonconductors, the general "law, we have stated, must operate, and opposite *electrical* "*states* must be excited in the *separated* particles. So far, "therefore, the *excitement* by *friction*, and the *excitement* by "*contact* and *separation*, appear to be referrible, in a general "manner, to the same principle. We shall now proceed to a "more particular *consideration* of the subject."

This theory difficult to prove.

3. To this *consideration* I shall soon come; but I must first observe, that it would be very difficult to prove that theory by ascertaining the effects of the *friction* in different points of the *rubber* and the *body rubbed*, in order to find out their *progress*. We see, upon the *whole*, that one is become *negative*, when the other is made *positive*; but nothing can indicate whether these effects are produced during the *contact*, or only at the

* Vol. XXVIII, p. 1.

sepa-

separation. Therefore the decision of this point must proceed from other phenomena, and Dr. Maycock affords me an opportunity of discussing this point by the passage which follows that above quoted.

Principal facts relative to excitement by friction, according to him.

" The principal facts (he says) relative to the *excitement* of " bodies by *friction*, may be expressed by the five following " propositions. 1. To produce *excitement* by friction, it " is essentially necessary that one of the bodies employed in " the operation be of the class of *electrics*. 2. If two *electrics*, " or an *electric* and an *insulated conductor* be employed, the one " body will, after the operation, indicate an electricity opposite " to that which is indicated by the other. 3. The effect of " *friction* performed with one combination of *dissimilar* bo- " dies is different from that which is produced by any other " combination. 4. The *friction* of two bodies, *similar* in all " respects to one another, produces *no excitement*. 5. If the " *rubber* of an *electrical machine* be insulated, only a very " slight charge can be accumulated in the prime conductor; " and, under such circumstances, the action of the machine " soon ceases altogether."

Errour in the distinction between electrics and conductors,

5. I shall first observe, that, had Dr. Maycock read my paper on the effects of *friction*, to which I shall here refer on many points, he would have seen the errour of the first electricians in their distinction of bodies, which he continues to admit, that of *electrics* opposed to *conductors*; as if the former only had the faculty to be *electrified* by *friction*. With respect to *electricity*, there is no other distinction than that of more or less *conductors*, which explains all the phenomena. From the property of *absolute nonconductors*, as are *resincus* bodies, whatever change is produced in the *electrical* state of their surface, either by *friction*, or by *communication* with an *electrified* body, it is not *propagated* on them; and this is their only distinctive property with respect to *electrical* phenomena. The difference, therefore, between these bodies, and the *imperfect nonconductors*, is this; that the changes produced on some points of the latter, either by *friction*, or by *communication* with an *electrified* body, are propagated on their surface, slowly on some, as *glass*, or almost instantly on the *best conductors*, such as *metals*.

all bodies conducting more or less: but on some the effects are not propagated.

Motion of the electric fluid

6. From this determination of the effects of the different *conducting* faculties of bodies, united with that of the nature of the

along different bodies.

of the electric fluid, which Dr. Maycock has not thought necessary to investigate, I derived in the same paper (pp. 3 and 4) the following theory of the effects of *friction*, which is to be compared with the phenomena. "The *electric fluid* resides "on all terrestrial bodies, every particle of *air* included; being "retained upon them by a mutual attraction, which, however, "differs in degree; some attract the *electric fluid* only when it "comes into *contact* with them; but then it *adheres* strongly "to the parts which receive it, or moves but very slowly along "their surface; which therefore are *nonconductors*: others "receive it at more or less *distance*, and it is *propagated* more "or less rapidly along their surface. *Glass*, though absolutely "*impermeable* to the *electric fluid*, permits it to move with a "sensible progress along its surface."

Theory of the effects of friction.

7. After these definitions of the *nature* of the *electric fluid*, and of its *motions* along different bodies, I thus define the effects of *friction*, connected with these premises. "*Friction* "excited between two bodies, has no other effect than that of "*disturbing* the natural *equilibrium* of the *electric fluid*, which "*equilibrium* tends always to be produced among all bodies, "according to its actual, but local (in a certain extent) quanti-"ties on them, and in the ambient *air*. If both the bodies which "exercise *friction* on each other are *good conductors*, the *equi-*"*librium* being constantly restored, this disturbance is not "perceived: but if one has more disposition than the other to "attract the *electric fluid* thus agitated, with the faculty of "transmitting it to its remote parts; when the bodies are "separated, either suddenly, or in general before the *equili-*"*brium* of the *electric fluid* is restored between them, one is "found *positive*, as having acquired a proportional quantity of "this *fluid*, greater than the *ambient air*, and the other *negative*, "as having lost that quantity." This is the theory of the effects of *friction*, which, in the same paper, I compare with direct experiments: but before I come to that comparison, I must explain the general plan of those experiments, and its motive.

Motive and plan of Mr. De Luc's experiments.

8. The obscurity which reigned on the effects of *friction* proceeded from a circumstance wanting in most of these experiments; they require the *insulation*, not of *one* only of the bodies, but of *both*, either *conductor* or *nonconductor*; else the

the whole of the reciprocal effect cannot be discovered. I had found this necessity by many experiments made with large bodies, with which I could exactly follow the motions of the *electric fluid*. But I could not suppose it easy for every experimental philosopher to procure this apparatus, which I had partly constructed myself; therefore I attempted to produce a small apparatus, containing in itself all the parts of the large one, which might easily be obtained by every experimental philosopher; and having succeeded, I thus introduced, in the same paper, this new plan of experiments on *friction*. "Mr. "Cavallo has given a table containing the results of his experi- "ments of this kind, wherein is found, that certain bodies "become either *negative* or *positive*, according to those by "which they are *rubbed*. *However, there remained to be known* "*what effect was produced on each of the bodies which exercised* "*that friction*. This has been one of the objects of my expe- "riments; for which purpose I kept *insulated* both bodies, "exercising *friction* on each other, applying *electrometers* to "both."

9. Then follows, in the same paper, the description of the apparatus with which these experiments were made: its figure, which is at the head of the paper, is half the size of the apparatus itself; and it may be seen, in that figure, that it is, in fact, a very small *electric machine*, with a *revolving part* and a *rubber*: but it is so constructed, that both these parts may be easily changed, for producing *friction* between different bodies, the effects of which are always shown by the *gold leaf electrometers*. I do not think it necessary to compare directly every part of these experiments with Dr. Maycock's theory; he is so intelligent, that, had he read my paper, he would have found himself those relating to the objects on which we dissent; therefore, I shall only indicate briefly some of these points.

The apparatus.

10. The fourth proposition of Dr. Maycock's theory, above quoted, is the following: "The *friction* of two bodies, *similar* "in every respect to one another, produces no *excitement*." This is the immediate consequence of his theory, but is contrary to mine: here, therefore, is afforded a criterion between them; and he might have found the decision in my paper. There, after having explained my theory,—that, in the *friction* between

Dr. Maycock's fourth proposition refuted by experiment.

between two bodies, which operation agitates the *electric fluid* on their surface, the body which is the most disposed to seize upon that fluid, and to transmit it to its remote parts, becomes *positive,* and the other *negative,*—I added : " This holds, not " only between bodies of *different natures,* but even between " the *same kinds* of bodies, if one be made to pass in *length* " over *one part* only of the other. This effect cannot be ob- " served with perfect *conductors,* as on them the *equilibrium* of " the *electric fluid* is instantly restored; but there is a known " experiment with two pieces of the same *silk riband,* in " which, by making one piece pass rapidly in *length* on *one* " *part* only of the other, the former becomes *positive,* by car- " rying off some *electric fluid* from the latter, which thus is " rendered *negative,* by losing that fluid."

Experiment with silk ribands,

11. These experiments I have repeated many times; by using pieces of wide and strong *silk riband* about a yard long, at the extremities of which were fixed proper pieces of wood, to keep them stretched; one being held very steady, while somebody made the other pass rapidly on one part of the former: then applying each of them instantly to the top of a *gold leaf electrometer,* the *riband* which has moved is found *positive,* and the other *negative.* I must observe, that this experiment cannot succeed, but when the air is very *dry,* commonly in winter, at the time that a divergence produced in the *gold leaves* by any cause is long preserved; else the effects produced on the ribands is soon dissipated.

with glass,

12. I have produced the same effect by the *friction* between other bodies absolutely *similar to one another,* namely, *glass* and *glass*; as may be seen in Exp. 3 of the same paper. The *revolving body* was a *glass* cylinder, and the *rubber* a piece of the same *glass.* Now, the *revolving glass,* as the *riband* which passed in *length* over the other, carried off some *electric fluid* from the immovable *rubber,* and immediately transmitted it to the prime conductor of the small machine; so that, at every revolution, the *gold-leaves* connected with it increased in *divergence,* and at last *diverged* much as *positive.*

and with dissimilar substances.

13. All the experiments related in that paper demonstrate the same theory concerning the effects of *friction*; but I shall only indicate them shortly, as the details may be seen in the paper itself. In Exper. 1, a *brass* rubber acting on a *glass* revolving

revolving cylinder, the *brass* became *negative*, and the *glass* was made *positive*. This is the same effect produced by a *metallic amalgama* laid on the rubber of the electric machine. In Exper. 4, a *sealing wax* rubber applied on the same revolving *glass* cylinder, the *sealing wax* becomes *negative*, and the *glass* is *positive*. The latter, as being a better *conductor*, carries off a greater part of the *agitated electric fluid*. In Exper. 5 is seen a very singular case. Having used for *rubber* a piece of *India-rubber*, on the same revolving *glass* cylinder, according to the degree of *pressure*, sometimes the *glass* became *positive*, and the *rubber* then was *negative*; at other times the former was *negative*, and the latter *positive*. This case shows, that, between the *same bodies*, when they have a disposition to *adhere* to each other, *friction* may have inverted *electrical* effects, according to the degree, or parts, that the adhesion takes place.

A metal, when insulated, rendered either positive or negative, according to circumstances.

14. I come now to very remarkable changes in the *electrical* effects of *friction*, according to other circumstances. It has been seen above, in Exper. 1, that a *brass* rubber, applied to the revolving *glass* cylinder, became *negative*, and the *glass* was made *positive*. But in Exper. 6, the same *brass* rubber being applied to a revolving cylinder of *sealing wax*, the latter was made *negative*, and the *brass* became *positive*. Thus, therefore, *brass*, though the best *conductor* as a *metal*, when it is *insulated*, and thus retains the effect produced on it by *friction*, shows, that it is rendered either *positive* or negative, according to the body which exercises friction upon it.

Sealing wax rendered positive by friction with certain bodies.

15. With respect to *sealing wax*, which is our common test to discover whether our *electroscopes* indicate the *positive* or *negative* state by their *divergence*; because *sealing wax*, when rubbed with the hand, or some cloth, becomes *negative*; exper. 7 proves, that *sealing wax* itself is made *positive* by *friction* with certain bodies. In this experiment, the same revolving cylinder of *sealing wax*, which before was become *negative* by a *brass* rubber, was made strongly *positive* by the *India-rubber*.

Other experiments, showing different effects on the same bodies.

16. Exper. 8 is farther illustrative of these differences of *electrical effects* produced by *friction* on the *same* bodies, according to those which exercise *friction* on them. The object of that experiment is one of the *India-beads*, the size and colour

colour of a cherry, used by Indian women in necklaces or other ornaments, which consist of an inspissated vegetable oil. One of these *beads* I made to revolve by a glass axis, and applied to it successively a *brass* rubber, and a *sealing wax* one: the *brass* rubber rendered it *negative*, and became itself *positive*; but the *sealing wax* rubber made the same bead *positive*, becoming itself *negative*.

Deductions from these experiments.

16. All these experiments prove, first, that the distinction between *electric* and *anelectric* bodies was illusory; that none, in their natural state, are either *positive* or *negative*. With respect to *friction*, these experiments demonstrate, that this operation has no other effect than that of disturbing the *equilibrium* of the *electric fluid* on their surface, one of which, according to circumstances, retains *more*, and the other *less* of that *fluid*.

17. If Dr. Maycock happens to see this abstract of the experiments contained in my former papers in your Journal, I think he may find, that every thing belonging to *electrical phænomena* is much clearer than he had imagined: he, however, encouraged natural philosophers to collect all the *known facts* under some *theory*, as tending *to advance our intellectual improvement*; and he will now judge whether I have accomplished this purpose.

Supposed obscurity with respect to the action of the galvanic trough.

18. The last part of his paper will lead us to another field, where he finds *much obscurity*, but on which I think light will appear. This part relates to what he calls the *galvanic battery*, saying: "that all the opinions, which have been proposed to "account for it, are unavoidably *hypothetical*, and indeed very "*unsatisfactory*; and that, therefore, every *fact*, which relates "to it, deserves *attention*, although its application may not be "clearly perceived." This gives me hope that he will consider what I shall here explain; expressing, however, again my regret, that he has not known my paper in your Journal on the *galvanic pile*, an apparatus in which the causes and effects may be easily followed; but I hope to make them clear, even in the apparatus of *troughs*, the only one Dr. Maycock seems to have used. I therefore shall copy first what he says of his experiments.

Dr. Maycock's experiments with it.

"I filled one of the new *porcelain troughs* with an *acid fluid*, "so that the *metallic plates*, and their connecting *arcs*, were

"*com-*

" completely covered. In this state, a *trough* of 10 *pairs of* " *plates,* 3 inches square, decomposed water very rapidly. " Anxious to know how far the division of the *trough* into " *cells* is requisite, I placed the *metals,* connected by the *bar,* " in a *trough* without *partitions,* and filled it with the same " kind of *acid,*—but no action ensued. The action which " took place in the first experiment appears *inconsistent* with " all our *theories*; and it seems not a little curious, since a " *communication* between the *cells* is not an impediment to " action, that no action was evinced in the second experiment. " It would afford me much pleasure, should these observations " call the attention of your readers to the theory of *electrical* " *excitement.*" It has certainly been the case with me, and I shall now explain how I find his experiments *consistent* with each other, and also with my theory.

Attempt to explain the apparent inconsistency in them.

19. In the first of these experiments, the *trough* with *partitions* produced a series of *ten* distinct *pairs* of the *two metals,* which, being formed of plates 3 *inches square,* were sufficient to produce the effect described; as the *liquid* was a *conductor,* which transmitted undisturbed the effect of each *pair* to the next on both sides; as does the *wet cloth* in the *galvanic pile.* But when the *plates* were entirely *immersed* up to the *bars* in the *liquid,* the latter being a *conductor* which embraced the whole, every difference between the *metals* in each intermediary *pair* was destroyed, and the effect was reduced to that of *one* single *pair.*

Apparatus to imitate the phænomena of the electric eel.

20. This will be shown by an analogous experiment, which, for another purpose, I made some years ago at Berlin, related in p. 253 of the 2d vol. of a work under the title of *Traité élémentaire sur le Fluide électro-galvanique,* published at Paris, in 1804. I had then in view the phænomenon of the *electric eel*; that fish which produces the *shock* while in *water.* I tried to imitate that *eel* by a *galvanic pile,* composed of 30 groups of *zinc* and *silver,* separated by pieces of cloth imbued with salt water. These groups were held together by 3 glass rods, so kept together as to leave no projection outwards, and resembling so far an *electric eel.* With this pile I made the following experiments:—1. It being held upright, I received a strong *shock* from it: having applied to it the usual *glass tube with water,* the *gasses* were produced in that tube. 2. I laid the pile on my

Experiments with it.

on my table; it continued to produce the *shock*. 3. I laid it in a narrow *wooden trough*, with a little *water* at the bottom; the *shock* was less. 4. I poured successively more *water* into the *trough*: in proportion as the *water* rose round the *pile*, the *shock* was less; and at last, when the *water* covered it entirely, not only there was no more *shock*, but, having applied between its extremities a *glass tube with water*, no *gas* was produced. The *electrical eel*, therefore, has no perceptible analogy with the *galvanic pile*, though the effects are similar.

This, I think, will show Dr. Maycock the manner in which his two experiments are *reconciled* with each other, and are *consistent* with my *theory*. It will also give me much pleasure, sir, if Dr. Maycock, finding any objections to my explanation, will transmit them to me through your valuable Journal; for I have a great regard for him, though not personally acquainted with him.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor, October the 5th, 1812.